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## **Community Ecology of the Central Gyre of the North Pacific**

Grant No. N00014-94-1-0315

Principal Investigator: John McGowan

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The book is about the pelagic community ecology of the central gyre of the North Pacific (see appendix). Chapter one includes studies of paleo-circulation, dimensions, early concepts and a narrative of our cruises. Other chapters in first draft form are on; community structure, co-evolution, biogeography and large scale biological-physical relations. Outlines and literature searches exist for chapters treating physical chemical structure, species spatial structure and co-occurrence over time, disturbances and resilience. Diversity maintenance theories are tested and comparisons to the California Current are made.

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## Outline

### Pelagic Community Ecology:

#### The Central Gyre of the North Pacific

#### Part I BACKGROUND

1. A little History  
Paleo-circulation and age of gyre and its assemblage.  
Early concepts and explorations, The Climax cruises narrative.
2. Community structure and why it's important. What is it?  
different concepts, co-evolution, competition vs disturbance. Architecture.
3. Why the Gyre?  
Biogeography, Sverdrup's water masses, Fager/Mcgowan, Venrick, Kaniya, sediment, boundaries/scale, circulation.

#### Part II THE PATTERNS

1. Physical/chemical structure, T, S, sigma-t, NO-3, NO-2, PO-4, SiO-3, vertical.
2. Seasonal and longer term physical.
3. Vertical phyto biomass and spp. + primary productivity.
4. Vertical zoopl biomass and spp. copepods, chaetognaths, euphausiids, fish.
5. Concordance - regroup in vertical.

#### Part III CHANGE and DISTURBANCE

1. Phyto diversity maintenance & persistence.
2. Zooplankton diversity " " "
3. Disturbances (i.e. physical microstructure, internal waves, time series, phase shifts.

#### Part IV SUMMARY

1. Review of tests of theory
2. The South Pacific data
3. Comparisons to California Current
4. Why things are the way they are?

## Prologue

### What This Book is About

This is a description of the structure of the community of organisms found in the open North Pacific, far from the influence of land and with little previous history of habitat disruptions or exploitation by humans (Fig. 1). While some commercial, long-line fishing has gone on in the central portion of the great Central Gyre it was not intense. Over the 15 year period when we studied it on 12 different cruises we saw no fishing boats of any kind. The sparseness of top level predators, like tuna, the distance from ports and the great depths are responsible.

In addition to the almost pristine environment, we chose the area because previous exploratory cruises through the area had indicated there was far less small-scale physical-chemical variability here...plankton patchiness was also far less than in other areas of the Pacific (Table 1). Since the system was not very noisy from a sampling viewpoint, it meant that important lower frequency changes would be easier to detect; that is, the signal to noise ratio should favor our study. Also the large spatial continuity of property variability (or lack of variability) indicated that an intense study of "structure" in one locale would be representative of a much larger area, perhaps even the entire biogeographical province. It also meant that the ecological system might approach the legendary nirvana of an equilibrium community. More knowledge of such a system would surely enhance our concepts of community function and resilience. But these were concepts to be described and tested by our study.

The gyre has deep blue, low productivity waters; it is the oceanic equivalent of a desert. One of our early critics (of which there were many) said the place was not worth study because "not much lives there and nothing ever happens." He was only half correct.

It was our objective to search for a pattern in the relative abundance and spatial array of the species present. The fact that the system was so quiet, so lacking in the striking variability found elsewhere, seemed to be an advantage in our search for the regulation of the pattern. Physical/biological perturbations should be easier to see in such a system of apparent stability. The patterns we had in mind were the spatial and temporal occurrence of species in the water column and in the distribution of inanimate properties on the same scale. In other words, we wanted to discover the architecture of the ecosystem. From this approach, species co-occurrence could be seen and the species trends, if any, in local habitat preference be determined. In addition we sought the patterns in species dominance structure and how it may change in time and space. Dominance and spatial array are important attributes of any community. The

presence of such patterns and their persistence is an indication of considerable evolutionary adaptation to the "conditions of life" and may be expected in an old highly evolved climax community in an old, physically tranquil, system.

Other similar community studies have been done, but not in the open ocean where everything is mobile and where there are no substrate structures such as rocks, logs or bits of bark to hide behind or soil to burrow into. Therefore, variations in physical water column structure must act as habitat heterogeneity in ordering the architecture and species structure of oceanic assemblages. Studies of terrestrial or benthic communities have shown that their patterns are strongly influenced by substrate and therefore their population regulation must differ in important ways from that of the ocean.

The biota of any part of the ocean have the enormous body size range of some seven orders of magnitude (Fig. 2). It was, and still is, impossible to quantitatively assess the abundances of this entire spectrum on a sustained and synoptic basis. Our study therefore is limited to those parts of the size spectrum that we could regularly and quantitatively sample with gear of known characteristics of reliability and error. The larger size fraction of life in the ocean, such as fish and squid, cannot be quantitatively sampled with nets and trawls because these animals can detect and avoid the nets. The very small size fractions were also difficult to sample because the methodology, at the time, was unreliable, very time consuming, or simply did not exist. It is still difficult to achieve the objectives of architecture and species dominance structure for the very small organisms.

Our size fractions (Fig. 2) consisted of over 200 species of phytoplankton, about that many species of invertebrate meso-zooplankton and 130 species of larval fish (which we used as surrogates for adult relative abundance). We also attempted to sample the smaller meso-pelagic fish on a regular basis with the Isaacs-Kidd mid-water trawl, but some aspects of mid-water trawl samples were, and still are, problematical.

The limited size fraction we studied may not be too great a disadvantage or bias in the development of our concept of the basic time-space patterns of community structure. Trophic levels tend to have organisms of characteristic sizes. Trophic theory indicates that changes in biomass at one or a few levels should lead to changes in other parts of the system because of the dynamic energy transfers between levels (i.e. size fractions) and on which the state of the system depends. Because of this linkage there should be statistically detectable relationships between variability at all levels (Lindeman, Fasham, and Odum).

In addition to the measurements of the flora and fauna we spent much of our time measuring the physical-chemical structure of our area. Usually these measurements were made very close in time and space to the biological ones. Both sorts of measurements went on around the clock for periods of up to 13 days when we were on our "stations".

There were severe problems obtaining support for what were, after all, expensive cruises. The expense came as a result of ship-time costs; the personnel consisting mostly of graduate students and marine technicians were relatively cheap even though they were the intellectual and physical core of the study, or as one retired admiral put it "our main battery". This sort of incongruous mismatch seems common in marine science. Because of funding gaps we were unable to conduct a true time series although our efforts covered about a 15 year span. There were many large gaps between seasons and years when we have no idea what changes were occurring (if any) in our community. While all four seasons were sampled eventually, there was no continuity of measurement between them. The kinds of powerful analyses done with real, no gap, time series are impossible here. Although we are fairly certain we observed an important decadal signal and a large shift from the summer of one year (1968) to the next summer (1969), and we think our community has a very persistent and stable structure, as compared to those, say, found in the California Current, some changes may have eluded us because of temporal gaps in our measurements.

Our studies are, for the most part, observational and are not based on extensive experimentation aimed at determining "processes". True process oriented, experimental work is very difficult in the open ocean and the classic protocols of, controls, replication and randomization are near impossible. In addition to describing the state of the system our observations can and have been used to test predictions and/or hypotheses about the nature of regulation of system structure and function, the maintenance of diversity and about the evolution of oceanic ecosystems,

Now that there are many more studies of pelagic ecology (JGOFS and GLOBEC for examples) done from very different viewpoints, it will be for the reader to decide if we have achieved our objectives.

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